

**APPENDIX**  
**Version – 2 (09/14/2003)**  
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*(editorial changes to version –1 (08/15) are printed in red)*

This Appendix is complementary to the Examiner's 35 USC § 101 rejection of U.S. Application No.09/513,768, as stated in the present Advisory Action. In addition to refuting Applicant's allegedly "*compelling experimental evidence*" (Amendment and Response to Final Office Action, paper no.17, dated 01/29/2003, page 9 of 65) for the existence of Applicant's alleged new form of hydrogen (hereafter to be addressed as *hydrino*), this Appendix particularly points out just a few of an overwhelmingly large number of errors in Applicant's Grand Unified Theory (GUT) of Classical Quantum Mechanics, provided by Applicant to the US Patent Office as a main support for Applicant's invention. Such a direct rebuttal of Applicant's *hydrino* hypothesis has been expressly requested by Applicant in his After Final Response, paper # 17, pg. 34/11.6-11 from bottom. In confirmation of the Examiner's § 101 rejection, this Appendix shows that the utility of Applicant's invention is *per se* incredible because its theoretical foundation as presented in GUT proves to be incorrect, whereas the 80 publication papers presented as attachments to Applicant's Response to Final Office Action have failed to provide valid experimental evidence for the existence of the hypothetical *hydrinos*.

As pointed out in the Examiner's Advisory Action, **NONE** of Applicant's "*compelling*" evidence are valid, simply because of the following reasons:

(a) They are not published in scientifically qualified (e.g., refereed) journals.

To this category belongs also Applicant's main support for the present invention, i.e., the *hydrino* hypothesis in Randell L. Mills' book titled "Grand Unified Theory of Classical Quantum Mechanics" (hereafter addressed as GUT), because it is not scientifically evaluated. Furthermore, conference proceedings such as Attachments # 42, 57, 61, 65, do not belong to refereed publications.

(b) They have not been peer reviewed (just submitted), so they do not (yet) have the credibility that peer reviewed articles have. To this category belong Applicant's Attachments # 2-5, 8-13, 15-23, 25, 26, 28-30, 34, 36, 37, 39, 44, 47 and 48. Because of their fundamental flaws, they are not likely to pass the peer review process. By the way, the terminology of Rydberg States used in # 12, 13, 47, 48, 49 is not justified, since it has been conventionally used for atomic states with very high quantum numbers ( $n \rightarrow \infty$ ).

(c) Speculating *hydrino* as explanation for experimental observation unrelated to and not necessarily caused by *hydrinos*, such as excessive line broadening, enhanced EUV emission, unfounded "*indications*" of hydride chemical bonding, etc. To this category belong Applicant's Attachments # 6, 14, 40, 41, 45, 46, 51-53, 56, 58-60, 63, 64, 67-77, 80.

(d) Speculating novel applications not based on reality, which have no chance of success simply because the underlying *hydrino* hypothesis is *scientifically flawed*, as will be discussed in more details in the next section. To this category belong Applicant's Attachments # 24, 32, 42, 43, 50, 57, 65, 73-77 and 80.

(e) Misidentifying spectral lines as having origins in *hydrino* catalized reactions.

For example, Attachment # 55 to Applicant's After-Final Amendment turned out to be *incorrect speculations*: Specifically, the spectral line at 304 Å is very well known in the art as a He-II line commonly observed in the solar spectrum, as shown in Ref. [1], Fig.3 & Fig.5, the latter also showing other lines at 374 Å and 456 Å claimed by Applicant. This single failure of misidentifying the 304 Å He-II line, claimed by Applicant as having its origin in a *hydrino* catalized reaction, is enough to render *all other speculations* of spectral lines presented by Applicant *deemed incredible*, i.e., Attachments # 33, 49 and 54. In this regard, Applicant's obvious mistake of claiming a He-II line as a *hydrino*-related line also casts serious doubts in other similar (mis)identification cases, as described below.

Specifically regarding the alleged role of resonance charge transfer involving transitions to *hydrino* states and ionization of hydrogen-like ions to explain enhanced EUV radiation and resonance broadening, as claimed in many of Applicant's papers, it must be emphasized that enhanced EUV emission and excessive line broadening do not need to be caused by resonance ionizations as speculated by Applicant, since there are plenty of other conventional, physically more plausible causes. Without evidence of electron density enhancement, there is no reason to believe that resonance ionization of hydrogen-like ions could be responsible for the anomalous broadening and EUV radiation.

Specifically regarding the alleged H(1/n) peaks observed in X-ray Photoelectron Spectroscopy (XPS) spectra as an "*evidence*" for *hydrino*-type bonding in hydride, as shown, e.g., in Applicant's Attachment #38, Applicant's specific identification

of photoelectron energy peak is more a speculation rather than fact, since there is no factually compelling evidence why these peaks should originate from *hydrino*-type bonding. From the fact that the alleged  $H(1/n)$  peak intensities are not stronger, let alone *significantly* stronger, than the Oxygen 2s peak shown in Fig.28, it is more plausible to conclude that these peaks are due to *unidentified impurities*. Oxygen in Si:H is an impurity, due to exposure of the sample to atmosphere, or to the dissociation of  $H_2O$  present in the “dirty” plasma used to synthesize the Si:H layer, as generally known to one of ordinary skill in the art. The former type can be easily etched away, as evidenced in [2] (Fig.1, spectra (a),(c),(e) vs. (b),(d),(f)). One of ordinary skill in the art would firstly regard any unidentified peak as impurity peaks, and perform various tests, such as etching, as done in [2], rather than looking for unconventional explanation. There is no guarantee that Applicant is ***not again*** making *unjustified* claims on impurity peaks, in the same manner as Applicant has previously misidentified the He-II line, which *should have been known* to one of ordinary skill in the art from the overwhelming data published by other authors and the general availability of helium spectral tables well known to one of ordinary skill in the art.

Since all the “evidence” presented in Attachments 2-80 belongs to at least one of the types (a) to (e) above, they *all* are deemed incredible, and hence, *invalid* as experimental proof for the existence of the hypothetical *hydrino*.

Specifically regarding Attachment #1, the NASA Phase-I Final Report evaluating Applicant's work on “*Blacklight Rocket Engine*” expressly recites, again and again throughout the entire report, the words “if the *hydrino* theory is proven

correct", meaning that Applicant's *hydrino* theory is given the benefit of the doubt, and hence, cannot be admitted as a valid basis for Applicant's invention.

Consequently, all the Attachments #1-80 have failed to provide compelling evidence to refute the examiner's previous § 101 rejection of Applicant's claims.

Specifically regarding Attachments # 46 and 53, there are **other** physically *more plausible* explanations for the anomalous broadening observed, e.g., pressure broadening [3] (due to high pressure within a hollow cathode, despite a low pressure discharge plasma), resonance broadening [3], microwave-field broadening (Blochinzew's satellites [4]), and many other broadening mechanisms [5,6] which are fundamentally different than Applicant's "*resonance broadening*" due to *hydrino* levels hypothesized in, e.g., Applicant's Attachment # 46. Thus, even if Applicant's *hydrino* hypothesis would be assumed as physically plausible, an explanation based on a mere hypothesis where there are plenty of other conventional, more plausible reasons, is highly speculative. Moreover, Applicant's *hydrino* hypothesis not only is highly speculative, but also physically incorrect, as it is based on fundamental misunderstandings of conventional quantum mechanics, electromagnetic theory and theory of relativity, as will be discussed in more details in the following:

1. Applicant's fractional hydrogen levels are postulated, not derived from first principle.

Applicant obtained the fractional energy levels of his hypothetical *hydrino* (GUT pg.197, Eqs.5.1 & 5.2) **not by deriving** from first principle as claimed, but as a

**postulate.** Nowhere in GUT is the fractional energy levels derived, but only argued, based on “non-radiative” nature of the hypothetical  $1/n$  quantum states.

2. Applicant misunderstands that all stationary atomic states are non radiative.

If Applicant’s definition of “nonradiative states” is to be followed, then **all** conventional stationary quantum states should be interpreted as “nonradiative”. The wave function for an electron in hydrogen is derived from the Schrödinger equation by separating the time dependent part of the wave function,  $\exp(-i2\pi Et/h)$ . Thus, the spatial part  $R(r) Y_{l,m}(\theta, \phi)$  is stationary, meaning literally, it does not change with time. Moreover, the measurable quantity is not the wave function itself, but the electron probability density, which is defined as the absolute-square of the wave function,  $|\Psi|^2 = \Psi^* \cdot \Psi \neq f(t)$ , which is not a time-varying function, since the absolute-square of the time-varying component,  $\exp(-i2\pi Et/h)$ , is real and constant in time, i.e., static. Thus, per Applicant’s own definition of “non-radiative” states (GUT pg. 113/line 3), a static charge density is trivially non-radiative. Consequently, all  $n \geq 1$  quantum states are non-radiative, because the electron probability densities are static. This stationary property is also a consequence of the uncertainty principle. Since  $\delta E$  is zero, we have  $\delta t = \infty$ , which means, the wave function is stationary.

3. Applicant misunderstands why excited states do radiate, but the ground state do not.

The previous interpretation is nothing else but a *conventional* interpretation of QM. Indeed, stationary states are supposed to be stable in time. The reason why a  $n>1$  state does radiate through a transition from an upper state ( $n_2>1$ ) to a lower state ( $n_1 < n_2$ ) is because the transition probability between the two states is finite. This transition probability,  $P_{21}$ , is defined as being proportional to the overlap integral between the two wave functions involved,  $P_{21} \sim \int \Psi_2^* (\mathbf{a} \cdot \mathbf{D}) \Psi_1 d^3r \equiv \langle \Psi_2 | \mathbf{a} \cdot \mathbf{D} | \Psi_1 \rangle$  with  $\mathbf{a}$  = polarisation vector of the emitted photon and  $\mathbf{D} = e \mathbf{r}$  is the electric dipole moment **operator** [5,6,7], where  $e$  is the electronic charge. Since the operator  $\mathbf{r}$  applied to an eigenfunction  $\Psi_1$  of the Schrödinger equation in space representation is simply equal to eigenvalue ( $=r$ ) times the eigenfunction, i.e.,  $\mathbf{r} \Psi_1 = r \Psi_1$ , the integral  $P_{21}$  receives the interpretation of an overlap integral between  $\Psi_2^*$  and  $\Psi_1$ . This transition probability determines the spectral line intensity [3,5-7]. Obviously, a transition involving the ground state  $n=1$  is only possible to the upper states  $n>1$ , which is not radiative but absorptive, meaning that the ground state  $n=1$  is non radiative because the Schrödinger equation gives no physically meaningful solution for  $n<1$ . Thus, Applicant's statement in GUT/lines 4-6, that "*the Schrödinger equation gives no basis why excited states ( $n>1$ ) are radiative and the 13,6 eV state (meaning the ground state,  $n=1$ ) is stable*", is principally wrong.

#### 4. Applicant misunderstands the most basic fundamentals of the QM theory.

Applicant is confusing the electron charge density, which is proportional to the probability density of an electron, with the electromagnetic wave generated by a

moving charge, which satisfies the Maxwell and Helmholtz equations, the latter usually expressed in terms of the Laplace operator (GUT/Eq.1.1). According to elementary QM, the probability density  $\rho(r,t)$  is defined as  $\rho(r,t) \equiv \Psi^* \cdot \Psi$ , but not as the wave function  $\Psi(r,t)$  itself. While  $\Psi(r,t)$  satisfies the Schrödinger equation, which contains a first order time-derivative,  $\partial/\partial t$ , it does not satisfy the wave or Laplace equation, which contains a second order time-derivative.  $\partial^2/\partial t^2$ . Thus, Applicant's expression for the probability density of a hydrogen electron (GUT Eq.1.9), can not simultaneously satisfy the Schrödinger and electromagnetic wave/Laplace equations. A direct proof of Applicant's mistake can be very easily demonstrated by inserting Applicant's postulated electron wave function  $\rho(r,t)$ , as given (only argued, not derived) in GUT, Eq. 7 on pg. 111 and Eq.10 on pg.112, into the Laplace equation, GUT Eq. 1.1 on pg.48, here using the conventional integral representation of the Dirac delta function (see [8] pg.480, Eq.A4-6) to calculate the first and second order derivatives, as generally mastered by one of ordinary skill in the art. Rigorously performing all mathematical operations, it comes out that Applicant's wave function  $\rho(r,t)$  is **NOT at all** a solution of Eq. 1.1 as claimed by Applicant throughout his *hydrino* hypothesis. That Applicant's  $\rho(r,t)$  is *neither* a solution of the Schrödinger equation is too obvious, since it does not contain the electron mass. This single mistake alone is already enough to disqualify Applicant's *hydrino* theory. Yet, a lot more mistakes are to be revealed next.

5. Applicant is mistaken in understanding and applying Haus's non-radiative condition.

Haus's condition for (non)radiative moving charge is derived through the **current** density  $\mathbf{J}$ , but not the charge density  $\rho$ . It is  $\mathbf{J}$ , which satisfies the wave equation with the Laplace operator, as recited in Haus's Eq.(3) [9]. In contradiction, Applicant's interpretation of Haus's condition states that the electron charge density  $\rho(\mathbf{r},t)$  satisfies the (electromagnetic) wave equation, as unambiguously recited in the preceding sentence to GUT/Eq.1.9, "*The **solutions** of the classical wave equation are separable*", then followed by an expression for  $\rho(\mathbf{r},t)$ . Obviously, Applicant's formulation of Haus's condition in terms of electron material wave is based on wrong understanding of both Haus's electromagnetic theory and Schrödinger's QM theory.

5. Applicant is confusing QM eigenfunction with QM wave function.

QM eigenfunctions  $\Psi$  satisfy the equation  $\mathbf{A}\Psi = A\Psi$ , where  $\mathbf{A}$  is an **operator** (e.g.,  $\mathbf{H}$ ,  $\partial/\partial t$  or  $\partial/\partial x$ ) and  $A$  is an eigenvalue, which is no other than a plain number or quantity. A wave function is not an eigenfunction (of an operator), but it generally can be expressed as a *superposition* of eigenfunctions from a complete orthonormal set of such functions (QM principle of decomposition). In GUT, pg.64, Applicant's formulation of a wave function of an electron with spin in spherical symmetric systems as being a superposition of two spherical harmonic functions  $Y_{l,m} + Y_{0,0}$  is basically wrong in *at least two aspects*.

Firstly, according to elementary quantum mechanics, such a wave function can not be an **eigenfunction** of the angular momentum operator, because the wave functions  $y_{l,m} = Y_{l,m} + Y_{0,0}$  do not form a complete orthonormal

set, the latter having to satisfy the condition  $\langle y_{l,m} | y_{l',m'} \rangle = \delta_{l,l'} \delta_{m,m'}$ . Applicant's  $y_{l,m}$  is definitely *not* an *eigenfunction* of angular momentum operator, because the product  $\langle y_{l,m} | y_{l',m'} \rangle$  results in  $\int \{ (Y_{l,m})^* Y_{l,m} + (Y_{0,0})^* Y_{0,0} \} d\Omega = \delta_{l,l'} \delta_{m,m'} + 1 \neq \delta_{l,l'} \delta_{m,m'}$ . In plain language, Applicant's  $y_{l,m}$  is not orthogonal to  $y_{l',m'}$ . Thus, it is incorrect to formally write  $L^2 y_{l,m} = l(l+1)(h/2\pi)^2 y_{l,m}$ , or  $L_z y_{l,m} = -i(h/2\pi) y_{l,m}$ , since it is generally known in the art, a superposition of eigenfunctions,  $y_{l,m} = Y_{l,m} + Y_{0,0}$  does not yield an eigenvalue  $A$  in an eigenvalue equation  $A\Psi = A\Psi$ , as demonstrated above, but an **expectation** value, defined as  $\langle A \rangle = \int \psi^* A \psi d^3r$  where  $A$  is the angular momentum operator, which – in coordinate representation — is given by ([8] pg. 205 Eq. 7-87)  $A \equiv L^2 = -(h/2\pi)^2 \nabla^2_{\theta,\phi}$  having an eigenvalue  $l(l+1)(h/2\pi)^2$ , or the angular momentum projection operator  $L_z = -i(h/2\pi)\partial\partial\phi$  having an eigenvalue  $m$  ([10] pg. 93 Eq.25.23). Thus, Applicant's representation of wave functions in GUT/pg.64 Fig.1-2, is incorrect, because Applicant's wave function  $y_{l,m} = Y_{0,0} + Y_{l,m}$  does not yield the *eigenvalue*  $l(l+1)(h/2\pi)^2$ , but instead, an *expectation value*  $\langle L^2 \rangle = \int \{ (y_{l,m})^* L^2 y_{l,m} d\Omega = l(l+1)(h/2\pi)^2$ . This is because  $L^2 y_{l,m} = L^2 (Y_{l,m} + Y_{0,0}) = l(l+1)(h/2\pi)^2 Y_{l,m} + 0 \neq l(l+1)(h/2\pi)^2 (Y_{l,m} + Y_{0,0})$ , and similarly,  $L_z (Y_{l,m} + Y_{0,0}) = -i(h/2\pi)m Y_{l,m} + 0 \neq -i(h/2\pi)m (Y_{l,m} + Y_{0,0})$ . Applicant's confusion between *eigenfunction* and *wave function* can be made further obvious by the following alternative argument: It is well known to one of ordinary skill in the art, if a wave function consists of a single eigenfunction (of an operator), its expectation value would have the same value as the eigenvalue. If  $y_{l,m} = Y_{l,m} + Y_{0,0}$  is a solution of Applicant's "*classical QM wave equation*", then  $y_{l,m} = a \cdot Y_{l,m} + b \cdot Y_{0,0}$  is also a valid solution, although it is not an eigenfunction but a linear

superposition of eigenfunctions. Thus, the wave function  $y_{l,m}$  can not possibly be an eigenfunction (of the angular momentum operator), but instead, must be a superposition of eigenfunctions. Consequently, the correct (ortho)normalized representation of the wave function  $y_{l,m}$  would bear a factor  $1/\sqrt{2}$  in front of each eigenfunctions, or, in a general case,  $a \cdot Y_{l,m} + b \cdot Y_{0,0}$  with  $|a|^2 + |b|^2 = 1$ , as usual, with  $|a|^2$  being the probability to find the system in the  $(l,m)$  state, and  $|b|^2$  the probability to be in a  $(0,0)$  state. This new wave function  $y_{l,m}$  yields an *expectation value* of  $|a|^2 \cdot l(l+1)(h/2\pi)^2$ , which is not a *single* eigenvalue, but an average value depending on the (thus far arbitrary) components of the vector  $y_{l,m} = a \cdot Y_{l,m} + b \cdot Y_{0,0}$ . This completes the proof that Applicant's  $y_{l,m}$  is not an eigenfunction of the angular momentum operator, but a wave function, defined as a superposition of such eigenfunctions. The above distinction between eigenfunction and wave function is purely mathematical. It thus applies to both wave equation and Schrödinger equation.

Secondly, Applicant's representation of an eigen (wave?) function in terms of two eigenfunctions,  $Y_{0,0}$  and  $Y_{l,m}$ , but only one of them having the time dependency, as recited in GUT, pg.61, Eq. 1.65a & 1.65b, is conceptually as well as mathematically incorrect, since both terms must be solutions of the same Laplace or Schrödinger equation, both of which are time dependent. The same rebuttal has been also raised in Ref. [11].

#### (6) Applicant misunderstands the Uncertainty Principle in QM

As a matter of fact, a superposition of two or more angular momentum eigenfunctions  $Y_{l,m}$  is just a manifest of the QM uncertainty principle. The angular

momentum eigenfunction  $Y_{l,m}$  itself is stationary with respect to the angular coordinate  $\theta$ , because each eigenfunction is sharply defined in angular momentum  $l$ , i.e.,  $\delta l = 0$ . As an equivalent of the uncertainty relation  $\delta x \cdot \delta p \approx h/2\pi$ , we here have  $\delta l \cdot \delta \theta \approx h/2\pi$ . Thus, a sharply defined angular momentum, i.e.,  $\delta l = 0$ , corresponds to a completely undefined angular position  $\theta$ , i.e.,  $\delta \theta \rightarrow \infty$ . However, by constructing an angular momentum “wave packet” made of a superposition of several, or many, eigenfunctions  $Y_{l,m}$ , we have a finite  $\delta l > 0$ , and hence, a finite  $\delta \theta < \infty$ . As a result, we now can “see” the electron wave packet at a definite angular position  $\varphi$  occupying an angular interval  $\delta \theta$ , exactly as displayed in Applicant’s Fig.1.2 in GUT/pg.63. *This angular wave packet visualization used to be given as a routine exercise for undergraduate students in physics to acquire a **correct** understanding of the uncertainty principle, when the author of this Appendix was teaching “Relativistic Quantum Mechanics” at the Technical University of Clausthal in Clausthal-Zellerfeld, Germany, in the 1970s.*

Therefore, Applicant’s explanation given in GUT regarding Fig.1.2 is incorrect, and so is also Applicant’s understanding of the uncertainty principle. The same judgement has been made by a number of other authors contributing to Ref. [12].

(7) Applicant’s concept of electron spin is incorrect.

Applicant’s representation of the electron spin function being identical to the angular momentum eigenfunction  $Y_{0,0}$ , as recited in GUT/pgs.61-66, Eq.1.61-65, is incorrect. In addition to its non-orthogonality, as proven previously, Applicant’s erroneous representation of the spin eigenfunction will fail to work properly in a matrix diagonalization procedure to calculate the magnetic and electric field

effects on energy level splittings and atomic transitions, because two basis vectors are missing, either of the angular momentum with  $l=0$ , or of the spin operator. On the other hand, application of conventional QM to hydrogen and helium atoms has been proven accurate in predicting the effects [5,6], which has been also experimentally verified to great accuracy by Doppler-free laser spectroscopy [7], all personally conducted by the author of the present Appendix.

Electron **spin** is an intrinsic property of the electron, completely independent of its angular momentum. In contradiction to Applicant's concept, the spin is not an external property like that of an electron orbiting an external atomic nucleus, but an intrinsic property, such as an eigen-rotation on its own axis. Therefore, correct representation must be provided by an angular-momentum-independent and complete set of orthogonal basis vectors, which is conventionally represented by the Pauli spin functions, **S**, more specifically as column vectors (1,0) and (0,1). This results in two simultaneous conventional Schrödinger eigenfunctions of energy and angular momentum,  $\Psi_+$  representing spin-up and  $\Psi_-$  representing spin-down, both forming a new set of eigenfunctions (eigen vectors, or spinors), as explicitly recited by the author of this Appendix in Ref. [6], Eqs. 4 and 5. In this case, the spin operator is not represented by the angular momentum operator, as postulated by Applicant, but by the Pauli spin matrices,  $\sigma_x$ ,  $\sigma_y$ , and  $\sigma_z$ , or alternatively,  $\sigma_+$ ,  $\sigma_-$ , and  $\sigma_z$  [5-8,10]. This two-component spinor representation can be further expanded to a four-component spinor formulation in order to render it Lorentz-covariant, from which a correct gyromagnetic ratio of the electron spin ( $g_s=2$ ) automatically follows ([13],

pg.25/lines 12-24), thus refuting Applicant's repeated allegation that conventional QM is not relativistically (Lorentz-)covariant.

(8) Applicant's hydrogen electron wave function is seriously flawed.

Applicant's formulation of hydrogen electron wave function leads to *self-contradiction* and is thus *incapable* of correctly predicting the multiplet intensities, the transition probabilities and their selection rules. The wave function in Applicant's classical antenna formula in GUT pg.144, Eq. 2.42 is incomplete for not including the radial function, rendering it incapable of deriving correct multiplet intensities or transition probabilities, because multiplet intensities and transition probabilities originate from the radial wave function. However, if Applicant's  $\delta(r-r_n)$ -like radial wave function is incorporated, Eq. 2.42 results in a selection rule proportional to  $\delta(n-n')$ , which is again incorrect, since  $n_1 \rightarrow n_2$  transitions with  $\Delta n \neq 0$  would then be prohibited. On the other hand, conventional QM is known to give accurate multiplet intensities, transition probabilities and their selection rules *simultaneously*, even under complicated Zeeman and Stark level splittings, as demonstrated in [5-7]. Applicant's failure to derive the correct multiplet intensities, transition probabilities and their selection rules, is a solid proof that Applicant's *hydrino* hypothesis is fundamentally wrong.

(9) Applicant's application of Special Relativity theory is incorrect.

Regarding Applicant's *relativistic length contraction*, described in footnote 2 in GUT/pg.55, it is to be *strongly emphasized* that an orbiting electron is **not an inertial system**, because we here have an acceleration in the radial direction.

Thus, applying the *special relativistic* formula of length contraction, as recited in Eq.1 footnote 2, pg.55, is **fundamentally not allowed**. Furthermore, according to classical electromagnetic theory, accelerated charge will always radiate. Consequently, the whole results of Applicant's lengthy derivation & discussion on the relativistically length-contracted "*distance on a great circle*" (in plain language, the circumferential length of an electron orbit) is incorrect, for being based on a wrong understanding of the Special Theory of Relativity. Therefore, Applicant's hypothesis, or postulate, of fractional energy levels in hydrogen, as presented in GUT pg.197, Eqs.5.1 & 5.2, is incorrect.

Applicant's erroneous understanding of the Special Relativity theory is further manifested in Footnote 2, GUT pg.56, lines 1-8 from bottom, stating that Applicant's "*relativistic length contraction*" results in  $t=2r/c$ , which is interpreted by Applicant as a confirmation of the electron charge density going *straight through the atomic nucleus*, in accordance with the radial electron wave function for the ground state hydrogen ( $n=1, l=0$ ), i.e.,  $R(r) \sim e^{-r}$ , which allegedly remains finite at  $r=0$ . This, however, is *doubly incorrect*, since the *radial distribution* of the electron density is not given by  $\sim R(r)$ , but by  $u^2 dr \sim R^2 r^2 dr$ , which becomes zero at  $r=0$ , as given in [8], pg.222, Eq.7-178. Thus, the ground state hydrogen electron remains strictly in an orbit away from the nucleus, in direct contradiction to Applicant's *hydrino* hypothesis.

## CONCLUSION

Just by considering the above few examples of Applicant's errors in formulating the *hydrino* hypothesis as a basis for Applicant's invention, it can now be concluded, the previous § 101 rejection of Applicant's invention is *proper*.

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